GLASGOW VETERINARY COLLEGE.—

Prof. Young's Introductory Lecture.

THE Introductory Lecture to the winter session of Glasgow Veterinary College was delivered on October 27th, by Dr. Young, Professor of Natural History, University of Glasgow. Principal McCall occupied the chair; and in addition to a large number of students, the following gentlemen among others were present :—F. W. Clarke, Esq., LL.D., Sheriff-Principal of Lanarkshire ; Professor Cleland, and Professor McKendrick, Glasgow University ; Dr. Moore, Blantyre ; Rev. David Brown, St. Enoch's Church, Glasgow ; Professors Knox, Cooke, McConnell, and Macqueen, Glasgow Veterinary College. The following veterinary surgeons were also present :—Mr. Dunlop, Belfast ; Mr. MacIntosh, Dumfries ; Mr. Thomas Campbell, Kirkcudbright ; Mr. Lindsay, Alloa ; Mr. R. Rutherford, Edinburgh ; Mr. McFarlane, Greenock ; Mr. Pottie, Paisley ; Mr. Houston, Paisley ; Mr. J. McAll, Govan ; Mr. Weir, Airdrie ; Mr. Weir, Glasgow ; Mr. Blackie, Bellshill ; Mr. Bryce, Stirling ; Mr. Brackenridge, Holytown ; Mr. Gardner, Helensburgh ; Mr. Neil, Dumbarton ; Mr. Prentice, Glasgow ; Mr. Mitchell, Cranstonhill ; Mr. Pollock, Parkhead ; Mr. Mitchell, Glasgow ; Mr. J. P. Macqueen, Glasgow ; Mr. Wm. Anderson, jun., Glasgow ; Mr. Currie, Glasgow ; Mr. Wyper, Glasgow ; Mr. Blue, Mearns ; Mr. Panton, Blairgowrie ; Mr. Constable, Inchture ; Mr. George Hill, Glasgow ; Mr. Peddie, Cathcart.

Professor Young's address was as follows :—When I accepted Principal McCall's invitation to address you to-day, on the opening of a new session of the Glasgow Veterinary College, I did so, because the opportunity was offered me at once of testifying my interest in the success of the institution, and of giving fresh evidence in favour of the unity of the medical profession, whether the practitioner has to deal with animals or with man.

Gentlemen, you are at various stages of those studies which, in their lowest aspect, will fit you to earn a living, and in their highest will enable you to benefit mankind by diminishing the evils with which they have to contend. If in former periods the "healing art" was the object of popular sympathy, it is "preventive medicine" which commends itself to the man of science, even more than to the philanthropist. The alleviation of suffering is that which engrosses the attention of the kind-hearted, but it is a higher form of benevolence which fixes its thoughts on the prevention of that suffering, and finds, in the process of alleviation, chiefly the material for investigating the evil to be combated. A recent writer, himself a member of the medical profession, waxes eloquent over the generous disinterested zeal of doctors, who testify in no niggard way their admiration for a man whose lifework it is to devise means of disarming disease. The generous feeling is doubtless there, but it is tempered by the knowledge that the evil successfully combated has its place taken by another formerly in abeyance. Dr. Pangloss, in Voltaire's immortal romance, declared it the best of all possible worlds when he was robbed and maltreated by the Turks ; and still held to his creed when, having escaped from their hands, he endured still worse suffering as the consequence of undue devotion to sensual gratification. The evil he had escaped was replaced by another, and the sum of happiness and misery was thus kept in a state of equilibrium.

But if it is impossible to anticipate, even from the highest triumphs of preventive medicine, the removal of disease from the world, it is possible to count on the protection of men and animals from general, widespread calamities, and to anticipate the time when men and animals alike will suffer only from individual ailments. The curse of the Middle Ages was the epidemics which from time to time decimated and demoralised provinces. We still suffer from our inheritance: we have outgrown some, at least, of the faulty conditions imposed on man by a civilisation more rapid than the progress of science, but we now live under reversed conditions, for the science of to-day is in advance of its practical application. You, gentlemen, are on the threshold of the profession at a critical time. We seem to have, almost within our grasp, the means of averting the Scarlet Fever, Measles, Smallpox, the Carbuncle, the Splenic Fever, and the many diseases which annually spread grief and ruin. Time would not suffice were I to attempt even a mere outline sketch of this great field of medical enterprise; but I may, with fair hope of success, try to lay before you the reasons drawn from recent medical research for asserting the unity of the profession, and for urging that the scientific truth should be converted into a practical one by the closer union, if not the amalgamation, of the schools of human and veterinary medicine. I have a further object in view—to point out, namely, the extent to which recent experiments, even when manifestly successful, have established a sound method of treatment, but have still left open for you an abundant field for study and experiment.

Historically, the oldest point of contact between human and veterinary medicine is Smallpox, limiting the phrase to scientific contact. For there are older *rapprochements* which rest on popular notions only, and these were as erroneous as, in some cases, they were brutal.

I need not go over the history of vaccination; for all know that it was discovered that vaccination from the pock developed in the cow protected the human subject from the disease Smallpox. You also know that recent inquiries have demonstrated the imperfection of the current Vaccinia as a protective against Smallpox; that Erysipelas and other signs of blood-poisoning have followed, in many cases, the use of the ordinary vaccine matter. In Dr. Cameron's paper, in a recent number of the *Fortnightly Review*, you will find satisfactory proof that the value of the current matter as a protective has undergone a depreciation capable of accurate estimation by the statistical method. He shows that the protective influence of vaccination has steadily gone down, and that the only means of obviating the consequences has been the repetition of the operation in individual cases. By a series of experiments carried on, I grieve to say, at the cost of men, it is now made clear that the transmission of vaccine matter through man is unsatisfactory, prepares for a mortality not anticipated by members of the medical profession, who, as a body, are singularly ill-informed in this matter.

The theory of vaccination is, that if a substance developed as a morbid product in a heifer is introduced into the tissues of man, a series of pathological changes takes place in man which runs a definite course, the poison being finally eliminated from the system, which is left free of any material whereon the same poison can work: this freedom lasting for a term of years. It has been proved that the Vaccinia or introduced poison works not by means of its fluid portion, but by means of certain solid particles therein suspended, and which may be got rid of by the method of subsidence; that is, they may be got to settle down, leaving a clear supernatant fluid, which is innocuous. For this character of the poison or contagium, the objectionable term, particulate, is employed. The Smallpox or Variola, and the Vaccinia are distinct, both in the kind and gravity of the symptoms they present, and it is not yet certain whether they are identical diseases, or only similar in the character of the substances on which they act within the body. Be that as it may, the fact remains that two things are necessary—a poison introduced from without, and a substance which that poison acts upon. It would appear that every human being is born with, or at least early acquires a stock of material on which Smallpox or Vaccinia will act, and it seems probable that every one after a time may reacquire this stock, rendering revaccination necessary, and in most cases the repeated operation succeeds. But if revaccination is not practised, and if the person catches the contagion of Smallpox, the reacquired stock differs from the primary or congenital stock, somehow or another, for the Smallpox is modified, is not as a rule so severe. It appears that if the lymph, or fluid containing this poison taken from vesicles, has passed through numbers of human beings, it is humanised to such an extent that it fails to produce, when introduced into the calf, the true Vaccinia ; it has lost its power. To this I attach much importance, as bearing on a question to be afterwards discussed regarding the true character of the poison particles. Dr. Thiele obtained a good Vaccinia by inoculating the cow with Smallpox virus, and asserted that he had got an equally good Vaccinia without the cow at all, but simply by diluting the variolous poison with milk. If this is so, there would be strong reason for believing in the identity of the milder and the severer form of eruptive fever, characterised by the development on all the surfaces of a vesicular eruption. But though man and the cow have thus a disease in common, it does not follow that the same disease exists in other animals. Indeed, it appears as if it does not exist ; for Sheep-pox, though so like, does not seem mutually transmissible. The liability of other animals to similar diseases is not yet fully investigated, and in this domain, you have before you abundant opportunities for study. From observation and experiment it is hard to say what results may not follow ; and if nothing else is gained, you may at least have the opportunity of recording differences of pathological action.

The results of vaccination in a disease so easily transmissible as Smallpox, and so apt to assume the epidemic character, could not fail to suggest the possibility of similar treatment for other epidemic diseases. But though the idea has vaguely floated in the minds of medical men, it is only of late that it has taken practical shape. I must refer you to a very copious literature for the details of the progress of this line of inquiry, the last clear statement of the main lines of the history being given by Dr. Cameron in his presidential address to the Health Section of the Dublin Social Science Congress this year. It is sufficient for my purpose to take a few cases, but I must urge on your attention, the fact that the greatest strides taken by preventive medicine have started from the study of diseases with which it will, in the future, be your special duty, as veterinary practitioners, to deal.

A very terrible disease, Anthrax or Malignant Pustule, the *Charbon symptomatique* or *la maladie de Chabert*, has been productive of serious loss to the agriculturist. Here let me interject the remark that the loss of the breeder is bad enough in itself, meaning, as it does, the paralysis of a great industry and the impoverishment of an important section of the community, but it also means peril to the mass of the people. Epidemic diseases do not burst suddenly into existence ; there is a period of incubation in each individual, and there is for the animals of an infected district a period of gradual development, during which the full meaning of the spreading ailment is uncertain. During this period the community continues to get milk and flesh meat from the agriculturist, and runs a chance of infection from the secretions and tissues of the diseased animals. It is an open question to what extent such food is a source of danger, to what extent cooking obviates the risk. The evidence is contradictory, but this very circumstance increases the importance of our thoroughly investigating every point in the history of a disease. When, therefore, I urge the duty of protecting the agriculturist, it is not merely on commercial grounds, but also in the interests of the health of the people. Human and veterinary medicine here overlap in such a fashion, as to make the existing separation between the students of these two branches of one great art more preposterous than it has ever been.

Now this *Charbon symptomatique* has been brought within the domain of preventible diseases by the work of MM. Arloing, Cornevin, and Thomas, veterinary surgeons at Lyons. They made careful experiments on animals, and demonstrated that the poison of this disease can be "vaccinated"—*i.e.*, can be introduced in small quantities into the blood, with the result of working up the second factor, so as to give the animal immunity from graver infection. This important—nationally important—result has been, by the French Government, put to the practical test, and the laboratory experiments of the gentlemen referred to will find their reward in the increase of wealth of the community, not to speak of the suffering annually saved to hosts of our domestic animals whose condition is, so far as disease is concerned, aggravated by the fact of their domestication.

But these results are only an outcome of the researches initiated by Pasteur, who has reduced prophylactic medicine almost to an exact science. If Helmholtz, a surgeon, was the first to demonstrate that the solid bodies in a fermenting fluid were the causes of the change, and, by filtering the fermenting substance, to prove that the fluid part had no effect, thus supporting La Tour's great discovery of the potency of the yeast germs, a chemist has amply repaid the surgeon's contribution to chemistry by opening up new fields of medical research. Pasteur was a chemist, but with the insight of true genius, he was able to grasp the essential points in the physiological problems accidentally brought under his notice in the course of investigations entrusted to him, in the belief that they were essentially chemical questions. It happened to him to investigate the Splenic Fever of cattle, and he has revealed the extraordinary and unexpected relations in which that disease stands to a very varied set of phenomena. It had been known for years that in the blood of animals affected by Splenic Fever, a schizomycetous fungus, commonly known as *Bacillus anthracis*, was found, and it was believed that this lowly organism was the cause of the disease. Pasteur, putting various facts together, showed that the disease was spread not merely from beast to beast, but was actually nursed by the very means taken to stamp it out. The carcases of the victims were buried, and the worms were the agents which brought the poison to the surface, whence, by the rank grass growing over the graves, it was retransmitted to the occupants of the field, the infective agents being assumed to be the *bacilli* above mentioned. He experimentally demonstrated the fact by feeding cattle over the grave of their predecessors, and, finally, after many experiments, cultivating the germs in organic fluids, such as meat juice, he obtained a fluid containing the particulate contagium. With this he vaccinated animals, proved that he could convey the most malignant form of the disease, or a greatly modified form which, so far from being fatal, affected to a comparatively slight extent, the health of the animal, yet left it safe even when the blood of animals dying from the worst forms of the disease was injected into their tissues. Confident in the results of his experiments, he boldly put his opinion to the severest possible test, and publicly inoculating animals, some of whom he had, others he had not prepared, foretold the results in each set of cases.

It is the most striking result of his well-devised experiments that Splenic Fever is now, under authority and at the cost of the French Government, treated as is Smallpox in Britain—warded off by a comparatively harmless vaccination.

A similar story may be told of the Fowl Cholera, for which Toussaint successfully vaccinated, using the *bacillus* found associated with the disease and cultivating it in various ways. Pasteur improved upon Toussaint's process, and communicated the disease in a mild form, which, however, was completely protective even against the crude poison got from fatal cases.

The Pig-plague was likewise shown by Klein to be communicable, he having cultivated the germs and used them for inoculation.

I need not go over all the diseases to which this mode of treatment has been applied, or is in course of application. All that I wish to point out, *hoc statu*, is, First, that these diseases of the lower animals seem to depend for their virulence on solid particles ; Second, that, as Pasteur has the exclusive merit of showing, the virulence can be abated without altering the character of the process so as to impair the protective influence of the vaccination.

All these results have been gained by experiments on animals, and not one of them could have been foreseen by any process of reasoning, or inferred even from the most carefully observed facts afforded by the chances of veterinary practice.

One other disease I must refer to before passing to the human affections to which parallel treatment may yet be found applicable.

Hydrophobia is a fever which runs a definite course when the period of incubation is ended. Pasteur is at present endeavouring to ascertain if the germ-bearing saliva may be utilised, as in the blood of Fowl Cholera, for purposes of vaccination, and thus to extend the practice instituted by M. Galtier, a veterinary surgeon of Lyons, of introducing for protective purposes, the saliva of Rabies into the veins of sheep and goats. This series of experiments is not yet complete, but there is reason to hope that that awful disease may yet be brought under control.

The diseases of man to which it is desirable that similar treatment should be applied are Scarlatina, Measles, Diphtheria, Typhus and Typhoid Fevers, Relapsing Fever, and Malarial Fevers.

It is right, however, that I should mention a case in which science has more than justified a popular prejudice. It is a common notion that Consumption is infectious, and there is consequently an objection to a healthy and a consumptive person occupying the same room. It has been proved that tuberculous matter may be introduced into the bodies of animals, who thereafter are found to suffer from the development of tubercle in various organs. Dr. Alfred Carpenter, in 1880, denounced the sale of the products or of the flesh of tuberculous cattle ; and at Dublin, Dr. Cameron emphatically renewed the warning, which was afterwards taken up and emphasized by the press. Inflammations in the course of these local tuberculous lesions have been found to yield microphytes, and what is essentially a disease of malnutrition seems to be also in a sense a germ disease.

We have evidence, obtained by rigid experiment, of the transmissibility of diseases, even of some whose danger was not suspected and could not have been demonstrated save by experiments on living animals. We have, further, evidence furnished by careful experiments that certain diseases may have their virulence diminished, and their danger as epidemics averted by vaccination analogous in its character to that which gives protection against Smallpox. No one can doubt that the domain of preventive medicine has thus been vastly extended ; nor can it be foretold, till actual trial has been made, what other diseases may be under control. There is a vast field for experimental observation before you ; and, if you enter on that field you have the satisfaction of knowing that the work of your predecessors has minimised the "blind trials" unavoidable in the earlier stages of all inquiries. The main lines of the investigation have been determined, and you have the satisfaction of knowing that unnecessary suffering is obviated and time importantly saved.

In the majority of cases the contagium seems to be particulate. What are the particles ? are they vegetable germs ? or are they particles of organic matter which either exercise a catalytic action on animal fluids, or multiply by so disturbing the balance of the blood as to give rise to a number of similar bodies? The difficulties of determination are illustrated by the fact that some have thought Klein's vaccine particles to be inflammation products. In other cases the contagion seems non-particulate, and, if Toussaint's experiments in the case of Fowl Cholera (reinterpreted by himself on hypotheses which reconciled his results with the germ theory) are confirmed, it appears as if a non-particulate fluid could afford protection against a disease usually treated as particulate in its contagium. Hydrophobia seems a parallel case, as I shall afterwards point out.

While, then, there seems no doubt as to the existence of morphological contagia, there seem good reasons for believing that other contagia are non-morphological.

The early history of the germ theory was that of a struggle between the advocates of spontaneous generation, and the upholders of the doctrine that all organisms are the descendants of progenitors. The latter class of writers had frequent difficulty in explaining phenomena without an appeal to the former hypothesis, and we find Burdon Sanderson, in such a strait, suggesting that bacteria may be the carriers of infection, yet be utterly incapable of originating the infection which they convey (*Med. Chir. Trans.* 1873, p. 354). Bastian naturally asked why one set of particles should require the hypothetical aid of another set of particles to carry them ? Tyndall has denied Sanderson's view, but Sanderson has not himself withdrawn it. We shall find afterwards that in essence this suggestion has some ground, and that, on the one hand, the bacteria are credited with the possession of some peculiar content of the ferment order, which, and not the organism, promotes change ; on the other, they are recognised as carrying along with them substances of high potence, to which they are entirely subordinate. This latter theory assumes, as Dr. Cunningham has well argued, that the constant association of bacteria with certain diseases is an argument against the bacterial influence, but strong proof in favour of the common character of the changes which at certain stages encourage bacterial development. Of heterogenesis or spontaneous generation, it is now unnecessary to speak. The controversy is at rest, not because the parties are convinced, but because the advocates of the spontaneous origin of living things have had their arguments and experiments practically refuted, the margin of possible error in the facts thus adduced being too narrow to base on it any rational hope of success.

As the evidence against heterogenesis accumulated, the presumption in favour of the existence of atmospheric germs was extended to the causation of disease, until it seemed as if the germs were, *in se et per se*, capable of producing any given disease, the climax being reached in the statement that the Smallpox germ produces Smallpox, the Splenic Fever germ Splenic Fever. The strange confusion which, in spite of (at least, the uncertainty of) the botanical evidence, thought the specificity of bacteria indisputable ; the curious reasoning in a circle which held that, because poisons were specific, the germs supposed to be therewith associated must be also specific, and the converse, will furnish to the future historian of pathology a puzzling study. But the leaders of the new school, always, as is usual, outstripped by their disciples, have for some time recognised that germs alone could not be held as the exclusive factors, and we consequently find that Lister, the ablest of our English inquirers, some time ago recognised the importance of a second factor. In showing the different phenomena which were associated with the same bacteria under different conditions, he showed the power of the second factor, the soil. Now it seems as if the admission herein implied were likely to go farther; for as the specific difference of bacteria is denied by some of those most competent to decide on such a question, and as the evidence in favour of such specific differences is on all hands admitted to be so delicate as to require the skill and experience at the command of but few observers, the importance of the second factor becomes proportionately greater. There is the alternative already referred to, not acceptable to the advocates of the germ theory, namely, that the germs are infection carriers; but even this obviously does not help to get rid of the second factor.

Among the current statements is one to the effect that germs are found within the body. Lister, and after him Tyndall, have shown that the deepest air in the lungs, *i.e.* air expelled by the most forcible expiration, contains no floating particles, and Lister thus accounts for the absence of bacterial symptoms in cases when fractured ribs have punctured the lungs, and thus let air into the pleura. By these experiments the germs do not get into the body through the lungs, do not get in directly from the air; howsoever they get in, they do not set up mischief in ordinary circumstances. So far as Listerism, or the theory of our modern antiseptic surgery (in plain English, cleanly surgery) is concerned, the only active germs are those which attach themselves directly from the air to the wounded exterior of the body. The latter seem, therefore, to be credited with vitality; the former seem to be admitted, *sub silentio*, as negative in their influence. To this I attach much importance. But, as certain germs are assumed to have travelled extensively through the body before they appear, as in Typhoid Fever, at the intestinal closed follicles, there is both suspended vitality and an extraordinary selectiveness attributed to bodies whose chemical composition, systematic position, and even individual characters are still among the unsettled questions of biology. But it has been denied that germs exist in normal blood, the rounded bodies called micrococci being due to the disintegration of white corpuscles, the same bodies, remark, which are called undoubted spores in disease. Taking into consideration the negative results as to the decomposition of fluids obtained by Pasteur and others, I will accept as a truism of which he did not see the full force, Koch's italicised statement, "I have come to the conclusion that bacteria do not occur in the blood, nor in the tissues of the *healthy* living body either of man or of the lower animals." The word *healthy* is the important word, and I may have hereafter to refer to this final judgment, which implies the possibility of germs entering an unhealthy body in some fashion which does not find its equivalent in health. The fact is, the theory of diseased processes under the influence of germs is based on experiments which, dealing with fluids in glass tubes, can properly have no analogue in the living body, *except in cases of traumatic infective disease.*

The soil, then, on which the germs are planted, or the second factor, to use Maclagan's phrase, is, I repeat, at least co-ordinate with the bacteria to which specific properties are credited. It is known that dilution of a poison weakens its virulence (though in some cases contradictory experiences appear to have been realised), and to this dilution is attributed the fact that some people suffer at once, others not till after an interval, others again escape wholly. Whether there is, 1, real dilution, *i.e.* mixture of air uniformly with the contagium, or, 2, patchy distribution, as in the bacterial clouds which Tyndall imagines, or, 3, partial destruction by oxidation, is immaterial ; the fact is, that the incidence of the disease is unequal, and that the escape of some persons is due to the absence in them of the second factor. The history of every epidemic abounds in such cases, and the very theory of vaccination rests on the production artificially of that condition which it is not illegitimate to assume as naturally existing. Were it otherwise it would be impossible to understand how any one should escape. Indeed the advocates of the germ theory—I do not mean the leaders, but the rank and file—have overproved their case, and denied to the tissues of the body the power of originating tissue change, without the stimulus of foreign organisms. Thus Burdon Sanderson assigns the inflammation which follows the subcutaneous injection of ammonia to the bacteria with which the advancing inflammatory ring is crowded. It would be logical, under these circumstances, to say that the crotalus poison, the result of tissue change, is due to the presence of bacteria which are found in the mouths of snakes as of other animals.

It is incorrect, moreover, to assume bacteria as the sole factors in the infarction of inflamed parts ; the altered density of the blood counts for something ; the disintegration under heat of colourless corpuscles yields the zoöglœa—masses on which Koch rests so much, while the broken-down red corpuscles are the source of the micrococci, which do mischief when bacteria do not put in an appearance.

The analogy of beer fermentation has been invoked as explaining why one small portion of contagium, little more than a particle, may originate, and rapidly, a large mass of morbid and morbific matter ; and it is claimed that nothing short of fungus multiplication offers a parallel case. But two cases may be cited, neither of which can be explained by fungus action, the one case outside, the other inside the body. Dr. Lyon Playfair (Mem. Proc. Chem. Soc. 1847, iii. 348) drew attention to the decomposition of oxamide, a case in which a minute quantity of acid suffices to reduce a large quantity of the substance. The oxamide in presence of an acid and water gives rise to ammonia which combines with the acid ; oxalic acid is thus liberated, which reacts similarly with another portion of oxamide, and so on till the oxamide is entirely converted into ammonia and oxalic acid. The other case is within the body. The poison of snake-bite, say of cobra or crotalus, kills in a very brief period, the disorganisation of the blood being as thorough as it is rapid. No one has asserted the poison to be particulate, and there is, as I shall show afterwards, good reason for believing it to be a pure ferment. The preparedness then of the subject for this contagium has to be kept steadily in view. Wherein it consists, it is difficult to say, any more than it is possible to say what is the physical condition of the blood on which the poison operates. But it is certain that no contagium, save that of venomous animals, acts without a considerable period of incubation approximately fixed at a maximum of three weeks.

Contrary to what might be expected, it is said that contagium diminishes as the bacteria increase, *i.e.*, as decomposition advances. Baxter notes the destruction of the specific virus of Smallpox by putrefactive change, and Klein says the microphytes of Enteric Fever are most abundant when the necrotic changes are most advanced; while Jaillard and Laplat, in 1864, showed that Splenic Fever blood was the more virulent the fewer the bacteridia. Detmers, by successive filtrations, got rid of the microphytes, and held their innocence of the fluid proof of the particulate nature of the poison. But as his prolonged filtrations were done in air, he had in fact oxidised the poison, and gives the proof when he says he failed in winter, which he seeks to account for by supposing he had not got rid of the micrococci. In some cases this has been ingeniously met by a distinction between bacteria and their germs or spores ; as if, like parasites, the spores could not be infective unless introduced into another organism. But this transference is actually made in the experiments which have proved the decadence. This is an instance in which experiments outside the body give erroneous impressions.

When the death of the germs by pure oxygen under pressure was referred to by Bert, Pasteur said that the bacilli were, on contact with the oxygen, converted into spores (*corpuscles-germes*), a remarkable transformation of curious botanical significance.

Grauitz, on the other hand, asserted that innocent microphytes may be made virulent by cultivation. It is difficult to understand this, save on the hypothesis that the disintegrating action of microphytes on organic fluids creates substance in these fluids which, inoculated with the microphytes, give rise to septicæmia ; and my reason for making this suggestion is that a perusal of many narratives of experiments leaves a doubt on my mind whether specific diseases have been with sufficient care disentangled from septic disease.* It has been proved that the bacillus contagium has been made virulent by being transmitted through young guinea-pigs, and as this kind of observation has been so frequently repeated as to be now a safe, as well as a probable acquisition, I claim it as an argument against the microphytal character of the disease conveyed. The value of a young animal as a multiplier of virulent power lies in the fact that the fluids of a young animal are more highly corpusculate than those of an older, that there is a more vigorous tissue change going which multiplies the virus without any reason existing for the belief that the microphyte displays unusual vitality. The rapidity of the cell changes that take place, and the multitude of corpuscles liberated, are the physiological phenomena misunderstood when *bacterial* infection is said to be intensified by aid of young animals.

When, on the contrary, virulence is said to be diminished by cultivation in milk, or an organic infusion, animal or vegetable, there is here clearly a dilution of the virus, for the "germs" are not asserted to multiply with the same speed in these artificial fluids as in their proper soil, which is the blood of a living animal.

I have insisted throughout on the necessity for the existence of a "second factor" in every case of disease, on a preparedness of the blood of the patient. I have elsewhere had occasion to urge this in the case of Typhoid Fever, and what I then urged with a view to prevent a blunder in hygienic theory on the part of our health authorities, I now repeat on scientific grounds. I some years ago insisted on Influenza being a phase of Typhoid when it occurs epidemically, and Dr. Henderson, of Helensburgh, has not made himself popular by setting forth something of the same opinion. My contention was that the poison of Typhoid, whencesoever derived, had no power unless the patient was prepared; and I illustrated this thesis by the analogy of the Seidlitz powder, in which the two ingredients come in contact in presence of water. You may have every gradation from a Typhoid fatal in a fortnight to a lingering Influenza with a diarrhœa oddly enough occurring in its course and not perhaps got rid of, at least not recovered from, for four or five weeks. If this germ theory is to be dominant, though it is after all an hypothesis, then sanitary precautions are apt to be relaxed ; air-borne germs are a kind of providential dispensation ; the milkman is a convenient scapegoat. Amateur legislators, with enough law to blunder in a not easily corrected fashion, hunt

* Pathologists or physicians have not been always associated with the experimentalists. I am the last to claim this line of research as the exclusive property of the biologist or medical man, but the physician's eye and experience are, unless guided by preconception, of importance in translating morbid phenomena into a commonly understood language.

down the vendors of a fatty fluid, and filthy overcrowded houses are restored with their occupants to the tender mercies of the Casbys, whom Dickens describes in "Little Dorrit," who are always provided with a convenient Parcks to grind the people and minimise the expenditure on their houses, and whom the authorities find it (we ratepayers fail to understand how) so difficult to compel to realise their responsibilities.

The reasons for insisting on the vast importance of the second factor are :—

1. That not all persons suffer from communicable disease.

2. That epidemics terminate when they have exhausted the number of the prepared.

3. That similarly the disease terminates in the individual when the material on which its contagium operates is exhausted.

4. That vaccination now and again discovers insusceptible individuals.

5. That in the diseases of infancy exceptional cases occur in which one disease seems in its operation to create, or at least leave ready the pabulum on which another works.

6. That all diseases of a contagious or infectious nature are not equally transmissible to all kinds of animals or to all animals of one group—Splenic Fever and Glanders being noteworthy cases.

7. That innocent germs exist indistinguishable from the so-called virulent forms and sometimes in the same individual, while the same microphytes are found (Liesering, 1860) in Pig Typhoid and in Splenic Fever.

8. That, according to Tyndall in his test tube experiments, there is no reason why air germs should be moistened by the fluids on which they alight, unless there is an affinity between their contents and the fluid with which they have come in contact.

9. That according to Tyndall the "preparedness of the germs" (suggested by Dr. I. L. Boydon some years ago) accounts for the varieties of the "protean Typhoid Fever" (Murchison), explained by ordinary observers as due to a difference in the source of infection, Tyndall's explanation implying that all the "germs" in a contagium are at the same stage of development.

10. That predisposition has been experimentally shown to be needed, as when Pasteur cooled his fowls with cold water before he could secure their inoculation.

11. That Lister, when he admits that the fluids of wounds may modify the power of germs to cause injurious changes, practically admits the secondary position of the germs relatively to the fluids of their host.

I do not put forward these reasons for controversial purposes only ; I urge them because they underlie a big question of public duty. If disease depends only on floating contagium, we may hold our hands, and, with one eye fixed on the ratepayer, the other on that powerful municipal factor, the house-owner and his agent, say with convenient fatalism, it is the Lord's will. But if you need two things to make a case of disease, a contagium and a substance in a man's body on which the contagium may work, then sanitary science (which is not yet a science, only a very accidental art), or, in safer phrase, preventive medicine, has a heavy piece of work before it—to use every means of avoiding that preparedness on the part of the public which I conceive to be half the battle in an epidemic.

I have not yet referred to the germs in their systematic relations. Are they microphytes? Crichton, I have already said, thinks that the reagents used in microscopy have to do with the development of phenomena which simulate vitality. Dr. Cleland has drawn my attention to experiments which show that increased warmth, increased above blood heat, causes alteration of the blood corpuscles yielding appearances very similar to those claimed as proofs of the microphytal origin of disease. The breaking up of the red corpuscles so as to yield refracting bodies indistinguishable from micrococci, the solution of the outer layers of colourless corpuscles so that their granular contents, held together by the protoplasm of the corpuscle, present all the appearances of what are called the zoöglœa-form by some writers, the elongation of red corpuscles under the cover glass as drying advances and pressure increases, till at last a bacillus-like object is under the eye ; all these are mere suggestions of the difficulties attending the solution of this germ problem. On a question of such extreme difficulty as the determination of objects requiring the highest power of the microscope, it is above all things necessary that competent observers should be in agreement, and that conjecture should be reduced to a minimum. Yet an ardent, but not overwise admirer of Cohn and the germ theory, says Cohn is the only person who could recognise the difference of the germs. The unconscious satire of this is a sad comment on the lack of the true scientific spirit in which so important a problem should be discussed. On the other hand, observers are found calmly asserting the presence of micrococci and their specific characters in the intestinal contents of mammals. Microscopists of experience and caution may be pardoned if they decline to enter the lists with observers so omniscient and infallible. After wading through a tolerably extensive literature, I confess that I have not got anything definite and free from doubt. It is easy to say, as has been said to me and others, that the whole thing is beset with difficulties. If a foxhunter is lost in a mist, he does not gallop straight ahead, with the chance of jumping into a quarry : he casts about till he hits on some clue, and if none avails he is content to await the light. Is the reputation of any man so important when set against the good of the race, that we must accept his crude hypotheses, and thereon build a pile of new, doubtful, and probably false pathology ? When, therefore, Pasteur says, as the *British Medical Journal* declares he said, that every one must be wrong who doubts the specific properties of germs, when one finds that the germs themselves are open to doubt, that the properties assigned to them are unproved, and that even the specificity of their effects is capable of another than the ordinary "vitalistic" explanation, one feels that the current theory is at about its maximum of sway, and that in a year or two more scientific methods will have a chance of guiding your studies without your caution being called ignorance, and your desire for evidence treated as a proof of imbecility. The germ mania is, like some other evolutional manias, one from which the healthy common sense of naturalists will speedily resile.

It is not easy, within the compass of an address such as this, to gather together the contradictory evidence as to the so-called germs. The so-called microphytes are—1st, Micrococci ; 2nd, Spores ; 3rd, Bacteria ; 4th, Bacilli ; 5th, Vibriones. Under each of these five heads we are told that we have innocent, septic, and specific forms. The recognition of these varieties is effected by a circular reasoning to this effect : a certain disease has certain symptoms : in the blood or other fluids of the patient you have a microphyte : if inoculation with the fluid containing these microphytes gives rise to the same symptoms, the microphytes have done it.

But another fallacy has been appealed to. It is said that if a certain treatment is successful, the thing treated must necessarily have been of microphytal origin. Salicylate of soda is successful in the treatment of acute rheumatism, and, behold, acute rheumatism is a bacterial disease ! Do the people who thus talk pretend to omniscience ? In the first place, do they know for certain the lactic and uric formations which go on in the body as the normal results of abnormal cell chemistry ? Is the relation of urea to uric acid one wholly dependent on the intervention of vegetable germs ? Do they remember that salicylate of soda, like all other germicides, acts by lowering the energy of all corpusculated fluids ?

If few bodies are free of such germs, how is disease of the rheumatic or gouty character (for the two cannot be pathologically separated) not universal? To the advocates of a dogmatic assertion regarding the character of rheumatic or gouty disease, it would be vain to recall the fact that salicylate of soda has been proved experimentally not to destroy the vaccine virus, and they might deem it uncalled-for curiosity if one asked whether rheumatic fever had been produced by inoculation. It may be said by some man of science, anxious to help his medical friends out of their logical scrape, Perhaps each kind of germ requires its own antidote. Now has any one shown the germs of acute rheumatism to be susceptible to the influence of the drug? Has any one explained why the drug is not successful in all cases of acute rheumatism? Has any one even shown that there is a distinctive microphyte in that disease? A therapeutic inference such as this suggests in very strong terms that physicians have forgotten, if they ever knew, the complicated character of tissue-change, and have ignored the fact that a chemical substance which alters the composition of the blood must necessarily alter the composition of the tissue reaction to which this altered blood contributes.

It is the fashion to ridicule Liebig's theory of pathological fermentation, and on *a priori* grounds to call in question the possibility of particles of organic matter so disturbing the equilibrium of organic fluids with which they come in contact that, without themselves multiplying, they give rise to the multiplication of similar bodies. The experimental inoculation of Tubercle has shown that if cheesy matter is introduced into an animal's body, Tuberculosis may be developed. I hope no one has seriously put Tuberculosis among germ diseases, microphytal diseases! I cannot at any rate find anywhere a diagnosis of the germs which give rise to this disease of malnutrition. If such microphytes have been demonstrated, if their potence has been experimentally placed beyond doubt, then the warnings against the use of the milk of tuberculous cows are almost unnecessary; there is less danger from the milk than from other noxious influences. But nothing of the kind has been demonstrated, and nothing has been proved against the possible catalytic action of which, I take it, artificial septicæmia is the experimental proof. The distribution of such poison is a difficulty to which I ask your attention, because it is a very serious one. On the one hand you have a notion that microphytes cause Typhoid Fever: on the other hand that microphytes are a consequence of that blood dyscrasis which the Typhoid poison originates. Sewer gas has been put out of court as the first factor; it has been wholly neglected as the cause of the second factor. What is the first factor? Even in Glasgow I think we do not hear that the effluvia of Typhoid evacuations do much to spread the disease; they have done nothing in that direction in the wards of the fever hospital, where, however, they are efficiently disinfected before entering the drains. But the inspection of dairies satisfied popular excitement by trading on this disproved notion, and amid all the cackle for supervision of the milk trade, no general demand has been raised for the compulsory inspection of every farm in the country, just as every house in a town is liable to inspection. Foul matters in the water used for dairy purposes do, with the aid of milk, just that, and no more, which foul water does without milk—originate Typhoid. The difficulty as to the life of Typhoid germs in water applies to them whether the germs are microphytes or albuminous products of waste cast off from the ulcerated closed follicles. There is no evidence on either side of this question, and you have here a wide field for experimental research, both of the chemical and pathological kind. I may, however, indicate one or two phenomena which illustrate the line of research I suggest. The blood of Splenic Fever, dried with its bacilli, is after a lapse of time still virulent. The bacilli are said to be the virulent factor, but as I have already indicated, the associated organic matter has equal claim to the evil reputation

Nor can this be easily disputed in face of the fact that the virus of a snake's poison gland is terribly potent even after the lapse of years, a promising naturalist having been killed in the Jardin des Plantes by an accidental prick from the fang of an old specimen of venomous snake. Bert killed spores by means of compressed oxygen, but failed to destroy the virus of a scorpion, or of vaccine matter, or with pus from glanders. Chauveau gives a critical case when he reports that the lambs of ewes which died of Splenic are proof against that disease ; the placenta filtered out the solid particles, and only the fluid part of the blood was inoculated in the lamb.

This brings me to the non-morphological poisons which are, to use D'Arcy Power's description, soluble in water, capable of passing through porous earthenware under pressure, difficult to pass through animal membranes ; are killed by 71° C. (159° F.) in solution, or 100° C. (212° F.) in air.

The first disease I would speak of under the head of non-morphological poisons is Hydrophobia, and I do so, not because it is a subject on which there is much certainty—rather the reverse, but because it is of very great importance in Glasgow. The relations of the dogs in this city, and the enormous excess of males, and the increase of Hydrophobia, point to this as a subject well worthy the most careful investigation by the experimental method. In a recent conversation with Dr. Carpenter, he mentioned several cases supporting the view that some of the so-called Hydrophobic cases were hysterical, that in fact the diagnosis of cases where the nervous system is so largely involved is frequently open to doubt. Now the virus of Rabies is supposed to be communicable by the saliva ; and Pasteur holds that the microphytes conveyed therewith set up the disease. But recent experiments with non-rabid saliva show that that secretion desiccated is poisonous. Vandyke Carter killed monkeys with the saliva of patients labouring under Spirillum Fever, or Relapsing Fever. Now the poison of the venomous serpent is a fluid secreted by glands, is a normal product of normal tissue-change ; it is derived from glands homologous with the salivary glands. If we find that human saliva by the ferment (as we may call it in the meantime), ptyalin, affects organic compounds, it would be difficult to deny that it may contain other ferments, in small quantities in man, in very high concentration in serpents. The case of Hydrophobia, then, is still an open one. Galtier, of Lyons, a city whose veterinary school is doing more for pathology than any other institution, is at present experimenting on this subject. He has found that the virulent fluid may confer protection against inoculation into the tissues, if it is introduced directly into the stream of a blood-vessel. The experiments are most interesting, and will, if satisfactory, throw much light on the particulate or ferment character of the poison.

Naegeli thinks the contents of microphytes may be the ferment which sets up active changes, and that the disease is thus not the result of changes initiated by the feeding of the microphyte. Lister has adopted the same view. Onimus dialysed the blood of Typhoid Fever, and inoculated with the fluid full of organisms on the outside of the dialyser, but found it innocent ; whereas the residue within was virulent. If the organisms had been specific, both experiments should have succeeded equally ; but the ferment or the particulate poison, not microphytal, was kept back. To meet this case we should be called on to add another test to our germs ; bodies apparently identical are not identical if, when present on both sides of a dialyser, they are poisonous on the one, innocent on the other.

Septicæmia, claimed as a germ disease, has been produced, not by the crude septic fluids, but by the inoculation of sepsin, a substance obtained by Bergmann, in 1868, from putrid matter ; a similar result having been, as already said, obtained by Panum: the two observations placing this substance, as a cause of disease, among the non-morphological poisons, or ferments proper.

Gentlemen, I am not opposed to the Germ Theory. I do not cavil at it. But I am opposed to the unhesitating acceptance of any theory which involves so much hypothesis, and a sudden and entire uprooting of our former notions as to even normal processes. There is need for further observation, but above all for the more vigorous application of chemistry to the investigation of results.

I accept the results hitherto obtained as most valuable ; for practical purposes many of them are unspeakable boons to mankind and to our domestic cattle. But successful treatment does not constitute the basis of sound pathology. If I have to-day succeeded in drawing your attention to the grand principle which should ever guide your practice, namely, that success in the treatment should make you doubtful as to whether you have grasped the real pathological condition with which you have to deal, I shall be well content. As the constant association of two phenomena is not necessarily causal, so the constant curative effect of a particular drug is not demonstrative that any theory you may have formed on the condition of the patient is correct.

In conclusion, the lecturer urged again the necessity for the combination of the schools of human and veterinary medicine as essential to the progress of pathological science, and most important for the study of therapeutics.

[Since the above was delivered I have learned, through the courtesy of Dr. Fleming, that I have done injustice to the English students of Tuberculosis. I have not altered my statements, but hope to return to the question shortly and atone for my defective information.—J. Y.]

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